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SANTA BARBARA • SANTA CRUZ

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BERKELEY, CALIFORNIA 94720

May 16, 1974

Dr. F. H. C. Crick
Laboratory of Molecular Biology
Hills Road
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Dear Francis:

Permit me to express my pleasure and admiration at your article in *Nature* for April 26. I hope you will let me make a few comments, because it is impossible to read what you have written without being aroused to a reminiscent mood, even though I am comparatively a newcomer who joined the big parade some years after it started.

I enjoyed what you said about Barry Commoner. Really, he should be the last person to complain about oversimplification, because he tends to enter and oversimplify the fields of other people, such as fertilizers, soil chemistry, pesticides, and toxicology. I presume he still does not believe in DNA. He seems to resent the fact that DNA is put together by enzymes. I gather that he thinks that this contradicts the Central Dogma.

Your remark about Fritz Lipmann getting the word from Chargaff brought back memories to me of the 20 years I spent in the New York area. There was always great camaraderie between the refugee biochemists, who formed a tightly-knit group. I can well imagine Fritz Lipmann turning to Chargaff as the fountain-head of information on nucleic acids. I remember a historic confrontation between Ochoa and Chargaff at a big meeting on molecular biology in New York in the spring of 1962, when Chargaff (with, as you have said "his customary insight") compared the genetic code to the cabalistic signs that tramps leave on the gate post of households whose occupants give them a free meal. Severo, in his reply, seemed slightly flustered. As I recall it, he said, "*I have known Erwin Chargaff for years....*".

Arthur Kornberg has been lecturing at Berkeley this week. It is always a pleasure to hear him, because of his intense and enthusiastic preoccupation with laboratory biochemistry. His delight in science always comes through.

My reaction to the structure of DNA became greatly increased when the poly-U experiment came out, and at about this time I realized that the whole story of evolution was buried in the sequence of bases in DNA. I saw that the classical evolutionists had suddenly become "the walking dead". I compared the various hemoglobin sequences in terms of what was then known about the genetic code, and in 1962, it was possible to count the numbers of single-base and two-base differences between codons for amino acids in hemoglobins. I remember counting them up while riding in a taxicab across New York. The figures didn't change much when the code was completed 3 or 4 years later. I felt that I had to disengage myself from my job and other obligations, and I have never regretted doing so.

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I recall with pleasure a meeting in 1963 that was attended by Pirie and J. B. S. Haldane. I gave a talk in which I displayed alleged sequences of the bases in hemoglobin genes. Pirie said there was no such thing as the genetic code, and that the composition of tobacco mosaic virus protein depended on the host cell and not on the viral RNA. Fortunately, his remarks were later printed. J. B. S. Haldane, on the other hand, insistently demanded that I give him my manuscript for publication in his *Journal of Genetics*, which at that time he edited in India. But it was not to be, for he died a few months later.

I have not seen you in ages, and I hope you are well. I am still trying to keep this place going, although we are very short of money. Our "alumni" are all doing well. Three of them are full professors in Japan, all at a young age.

I am having fun with an argument about Mars. Lederberg, Sagan and Danielli think it would be dangerous to bring back a sample of the soil from the surface of Mars without cooking it practically to a dull red heat first. I am less pessimistic.

With best regards,

A handwritten signature in cursive script, appearing to read 'T. H. Jukes'.

Thomas H. Jukes